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Predicting Good and Bad Scores in a Clairvoyance Experiment: A Preliminary Report*

GERTRUDE RAFFEL SCHMEIDLER

Many of the experiments from psychological laboratories that are concerned with ESP have given evidence for the existence of telepathy or clairvoyance. But sometimes the results are negative; and often this occurs when the experimenter seems to have been biassed against ESP in the first place. Presumably the experimenter is an honest man, who would not knowingly alter his data in the direction of his bias. Yet it is possible that he unconsciously would select conditions which on the surface were unimportant, but which actually influenced the subjects so that they produced low scores.

The experiment to be described is an attempt to show the importance of such conditions. The subjects were divided into two groups, one of which was expected to get results of chance or lower; the other was expected to score

* This experiment was conducted for Dr. Gardner Murphy, who is Hodgson Fellow at Harvard University. His advice determined both the topic and its present form; and his criticisms and suggestions have guided the course of the research.

above chance. "Objective" conditions were the same throughout, but social atmosphere was not. Most of the differences in procedure between the groups were apparently so trivial that they might not even have been mentioned in another report; and yet here they seem to have determined the presence or absence of ESP.

Procedure

Points Common to Both Groups

Two experimental rooms were used; the subject sat in one and the stimulus cards were in the other. The shortest distance from the subject's chair to the cards was 42 feet, on a line which passed through the walls of two intervening rooms and a corridor. One of the experimental rooms was my office, a corner room with two windows, which was bare but not unpleasant. The other was a "dark room" planned for visual experiments. There were no windows in it, and the walls and ceiling were painted black. The paint was peeling away in places, and it held an accumulation of papers and of dusty bits of apparatus left over from earlier experiments. It was lit by two unshaded electric light bulbs.

The usual ESP cards were used, arranged according to a table of random numbers which is approved by statisticians for such purposes. The cards were prepared by an assistant, who first noted the order on prepared forms, and then made up decks to correspond. Each list was numbered. Thus there is a permanent record of the stimulus cards.

The subject was first shown sample ESP cards, and told that he was to guess a deck of 25 of the cards, arranged in random order. He was seated before a table on which were record forms, a pencil, his instructions, and the sample cards, as well as a telegraph key, which he pressed at the end of a run. I then left him alone, with the door shut, and waited in the other experimental room for the signal that he had completed 25 guesses.

During the time the subject was guessing, the stimulus deck was lying face down in the other office, next to the list which described it. Neither the cards nor the list had

been seen by the subject before his guesses; nor did I know what they were at the time when I saw and spoke to the subject. No one who knew the procedure thought it possible that the subject was guided in his guesses by any sensory cues as to the order of the cards. Further details of the method appear below.

A maximum of one hour was set for the experimental period.

Scoring was done by me, and checked by an assistant. It seems unlikely that many errors escaped us both.*

Points in Which the Two Groups Differed

The most important difference between the groups was in their own expectation of what their scores would be. There are some people (and they seem to be particularly in evidence among psychologists) who are firmly and blindly opposed to the possibility of psychic phenomena. One of them has recently told me, "If it were true, I wouldn't believe it." Now it is intuitively clear that a person who resists ESP so strongly should do badly at it, even though he *says* he is willing to cooperate in the procedure. In many experiments such skeptics would be included among the subjects who were expected to score above chance; and this might reduce the scoring level of the group as a whole. In this experiment the people who "knew" beforehand that ESP could not occur were kept in a class by themselves; and their scores contrasted with those of the more open-minded subjects.

But even the skeptics showed considerable variation in their attitudes. Some were profoundly uninterested in the whole problem, but willing out of simple good will to give me an hour of their time. Others were more emotional; one especially was eager for personal reasons to prove that psychic phenomena did not occur. It is my guess that on the whole such emotional attitudes are apt to result in scores well below the level of chance. Lack of interest will prob-

* A third check is now being made and will be completed before Dr. Schmeidler's final report is presented.—*Ed.*

ably result in chance scoring; and members of the A.S.P.R. would be predicted to score above the chance level.

There are other complicating factors, such as always enter into each session to make it unique. If the subject is self-conscious and trying too hard, if he has a hangover, or a seminar report due for that evening, or a bad cold, his scores may go down. If he has quiet self-confidence and is in a good humor, his scores should go up. But taking, by and large, the subjects who think that ESP may or does occur, as opposed to those who think it cannot possibly happen, I predicted scores above chance for the first, and at or below chance for the second.

Having subjects with favorable or unfavorable attitudes to begin with, I tried to accentuate those attitudes, within normal experimental limits. Both my rooms were generally considered quite suitable for research; so I put the skeptics in the "dark room" and their cards in my office, reversing this for the open-minded group. There was a battered laboratory table in the "dark room"; I left it there for the skeptics to work on—and as often as not, supplied them with the stub of a pencil, not overly sharp. For the other group I used a table with a fairly nice surface and set it under a window so that they could look out at Harvard Yard. Pencils were always freshly sharpened. There were cigarettes and an ashtray available; sometimes I offered them candy. Probably none of these differences mattered very much to the subjects (with the exception of the candy!) and yet they may have served to establish something of the atmosphere of dullness and drudgery on the one hand, and friendly cooperation on the other.

When a skeptic finished a run, he pressed a telegraph key which flashed a small light in the room where I was waiting. I then took away the deck he had been guessing; put the next one on the table; and pressed the corresponding telegraph key, to signal that he could begin another run. The signal in his room was a rather raucous buzzer.

With the more docile group, there was a longer interruption after each run. As soon as a subject signalled that he was done, I would go in to him with a list of the cards

he had been guessing. We would go over the scores together, discuss the methods the subject used, his impressions of success or failure, or anything else we wanted to talk about. This took up so much time that there were never more than ten runs in an hour; whereas in the same period there were fifty runs with the other procedure.

Results

The results came in pretty well as it was predicted they would. Sometimes the skeptics (called the "goats") scored above 5, which is the score expected by chance; and sometimes the open-minded subjects (or "sheep") scored below 5; but after a few weeks of experimentation the mean for the former group was 4.9 and for the latter was 5.4. A statistical test showed that such a difference, small as it may seem, would occur by chance only three times out of a hundred—in other words, that the odds are 97 to 3 that the goats would again do worse than the sheep if the experiment were repeated.

The logical next step was to repeat the experiment, to make sure. When I did, the sheep did less well, averaging only 5.2; readers of this JOURNAL will recognize the Midas touch.* The goats again had an average score of 4.9. The same statistical test applied to the data—now twice as numerous—shows that the odds are somewhat better than 99 to 1 that skeptics would again get poorer ESP scores than open-minded subjects under these conditions. The experiment is now being done still another time. Table I shows the actual figures for sheep and goats.

Describing only the averages omits some of the most interesting points of the research. The following details partake more of the nature of hints than of established facts. But there were dozens of such hints, some weak and some fairly strong; and collectively they may suggest some of the factors which operate in clairvoyance.

Take the subject who vehemently resisted ESP but was consciously trying to cooperate and to guess the cards

* See "The Midas Touch in Psychical Research," appearing in the April, 1943, number of this JOURNAL, and "A Further Report on the Midas Touch," this issue.

Table I

Analysis of scores obtained by skeptical and open-minded subjects.

	Open-Minded Subjects	Subjects Who Expected to Score at Chance
N (in decks)	257	376
Deviation	+84	−35
Mean	5.327	4.907
C. R.	2.62	−.90
P	.01	.18
$Mean_1 - Mean_2$	.420	
Sigma Diff.	.165	
C. R.	2.55	
P	.01	

right. While she was doing the experiment she imagined a deck of cards being flipped over, and wrote down each card as she "saw" it. That is not an uncommon description; many good subjects report similar visual images. Was it only accident that her score for fifty runs averaged 4.46, well below chance?

I think not. The picture of what actually went on is surely a complicated one; and it seems to me that at least these three factors must be somehow involved. (1) Her mother and grandmother seemed to have psychic powers. Perhaps she is potentially a little better at ESP than the average person. (2) She hoped to get low scores, to prove that ESP cannot occur; and therefore directed herself, unconsciously, *not* to get the cards right. As a result she overshot the mark, with scores averaging worse than chance. (3) On the conscious level, she considered it her duty to cooperate as a subject, and to obey whatever instructions an experimenter (no matter how misguided) would give her. Therefore her conscious experiences were the same as those of the other subjects who really were cooperating.

Certain subjects who were classed with the sheep also got low scores. By now I think I can spot one type which

is likely to do badly: the boy or girl who is sincere, shy, and self-conscious—who tries too hard. My guess is that the very good student will do badly too. If he is in the habit of pushing himself to the utmost, his attitude is too intellectual for ESP. And another variation on this theme: the tense person who is so self-critical that he seldom acts spontaneously, but always tries to think a problem out before committing himself on it. He is apt to find himself trying to reason about his guesses; and when he does, the scores usually drop below chance.

One negative finding still piques my curiosity. Previous research gives a great deal of evidence, both direct and inferential, to show the importance of a subject's mood in getting good scores. And yet when my subjects rated their attitude, during each run, as to interest versus boredom and enjoyment versus dislike, there was no relationship at all between their interest (or enjoyment) and success.

Yet even with them I am sure that a relationship exists. Scores on the first run of a session averaged higher than for all the other runs combined; and probably just on that first run a subject's interest and curiosity are keenest (See Table II). The unfortunates with examinations scheduled

Table II

Comparison of first deck called at a session with all other decks.

	First Deck called at a Session, All Subjects	All Remaining Decks
N (in Decks)	37	596
Deviation	+27	+22
Mean	5.73	5.04
C. R.	2.21	.05

for later in the day did not do too well. Some of the boys made their best scores after eating candy; some of the girls after being given a chance to talk about themselves. Can it be that they themselves do not know when they are inter-

ested and contented? Or is there some subtle difference between the surface mood they describe and a deeper emotional readiness to do well or to do badly? The point may seem a slight one; but in my opinion it is worth further study.

The experiment is still under way; and no formal conclusion can be in order. But both the quantitative data and my own impressions from watching and listening to the subjects point in the same direction, implying in the first place that clairvoyance occurs, and in the second that it is related to a variety of psychological factors which may facilitate or hinder it.

A Further Report on the Midas Touch

ERNEST TAVES, L. A. DALE, and GARDNER MURPHY

In the April, 1943, issue of this JOURNAL two of the present writers reported experimental ESP results which were characterized by various decline effects (1). These effects we attributed to a "Midas touch in reverse" on the part of the subject, the experimenter, or both. It was shown that when subjects called a series of decks of ESP cards, scores were significantly above chance for the first three decks, descending to the chance level thereafter. This effect we attributed to the Midas touch in respect of the subject: as the subject becomes less naïve for a given experiment, he loses his ability to do above-chance scoring. We also presented evidence in support of the view that the Midas touch may work in respect of the experimenter; the present paper, however, does not deal with this aspect of the problem. We want to stress the fact here that the decline phenomena mentioned above were not *a priori* expected in the data in which they were found. On the contrary, the experiments reported on in the first paper had been designed to test an entirely different hypothesis. But the Midas touch effect became so obvious that it could not escape detection.

On the other hand, decline effects *were* expected to appear in the data about to be presented, and the entire experiment was specifically planned to facilitate their emergence. The question is, Does the Midas touch hypothesis possess predictive value? Our results lead us to believe that it does. This new experiment will now be described.

Experiment BA

Subjects: It was necessary, of course, to use naïve subjects in this experiment, since it is supposed that the Midas touch makes itself apparent in the first few runs made by a given subject. Accordingly GM* asked Dr. B. F. Riess

* The entire experiment was carried out under GM's direction, and was made possible by moneys available to him from the Hodgson Fund of Harvard University.

of Hunter College if he could make an appeal for volunteers in the latter's psychology classes. Permission was kindly granted and early in March, 1943, GM addressed the classes and asked for volunteers for a "psychological experiment." The parapsychological nature of the experiment was not mentioned since it was desired that the subjects be as naïve as possible. A total of 45 volunteers was accepted and appointments were scheduled. All of the subjects, therefore, were girls of college age. None of them had previously taken part in formal ESP experimentation.

Methodology and Procedure: Three methods of calling were used in the experiment. Two of these are the AE and AJ methods described in another paper appearing in this JOURNAL (2: pp. 17ff. and also 1: pp. 71-72); the third method is similar to method AE, except that the subject *does not* receive knowledge of results after each individual guess. The letters "wkr" are used to signify "without knowledge of results," so that the new method is called AEwkr.

Each subject was used for three sittings, each of which lasted from fifty minutes to an hour and a quarter, depending on the individual subject's preferred speed in calling. At each sitting the subject was required to call nine decks of standard ESP cards: three decks by one method, three decks by another method, and the final three by the third method. Each subject always did a group of three runs without the experimenter's entering the room. At the conclusion of a group of three runs, and before going on to the next method, the experimenter discussed the scores, and other matters of interest, with the subject. The order of methods used was rotated throughout the three sittings; that is, 15 subjects in their first sittings used the methods in this order: AE, AJ, AEwkr; 15 subjects followed the order AJ, AEwkr, AE; finally, the last 15 subjects called in the order AEwkr, AJ, AE. For each subject, in her second and third sitting, these orders were rotated so that each girl began one sitting with each one of the three methods. We planned this rotation of tasks so that a direct comparison of the methods themselves could be

made. Three methods were used, moreover, in order to give the Midas touch ample opportunity to appear. A subject who has called, say, AJ, may still be considered naïve for methods AE and AEwkr. According to the Midas touch hypothesis, we expected that the *first run of all* (first sitting) would give the highest average. We also expected that the first three runs *of each method* would be above chance, and that the first run of these would be higher than the second or third. It would follow from this, therefore, that a subject would do better at her first sitting than at her subsequent sittings. It was for this reason that we had each girl come for three experimental sessions. These sessions took place at approximately weekly intervals.

LD acted as experimenter throughout the series. The objective conditions of experimentation (distance between subject and stimulus material, preparation of stimulus material, mechanical recording, etc.) were exactly the same as described in a former paper (1: p. 82). The subjects were paid on an hourly basis at the rate of seventy-five cents an hour. In addition, rewards were offered as below:

SCALE OF REWARDS

Score	*Reward*
8	.15
9	.25
10	.50
11	.75
12	1.00
13	2.50
14	5.00
15	8.00
16-25	15.00

This scale of rewards was conspicuously posted on the subjects' panel of the machine, and in most cases LD called attention to it before the subject began her first sitting. When the subject appeared at the office of the A.S.P.R., and after the completion of each group of three runs, the experimenter conversed with her on topics of interest. A real effort was made to create a friendly, informal atmos-

phere; cigarettes and candy were offered, and appreciation was shown for "high" scores. It is impossible to measure, of course, to what extent this attempt to create a "favorable" atmosphere was successful. Most of the subjects, however, said that they enjoyed the experiment and expressed eagerness to participate without remuneration in future experiments. Nothing was said, except in a very few cases, about "chance expectation," and the subjects seemed for the most part naïve on this point.

Results: In the entire experiment 1215 decks of ESP cards were called by 45 subjects—a total of 30,375 individual trials. A plus deviation of 51, giving a mean score of 5.04, was obtained. This over-all result is without significance.

The average score of the *first run of the first sitting* (that is, when the subjects were at their most naïve) was 5.42. Since there were, of course, only 45 "first runs" this value is not in itself statistically significant, but it is in line with the hypothesis stated *a priori.*

In Table I are presented the deviations obtained for the *first three* and *second three* runs called by each method (see 1: pp. 72 ff.).

Table I

Deviations of *first three runs* compared with deviations of *second three runs* (by each method) called by naïve subjects, Expt. BA.

Runs	*N (in decks)*	*Dev.*	*Mean*	*Dev.*	*Mean*
1	135	+40	5.30		
2	135	+ 6	5.04	+40	5.098
3	135	— 6	4.95		
4	135	+ 4	5.03		
5	135	—26	4.81	+ 2	5.005
6	135	+24	5.18		

It will be seen from the above table that, as predicted, the average score of the first run for each method is higher (deviation = +40) than the average score for the second (+6) and third (—6) runs.

None of the three methods used yielded a significant mean score; the obtained means for these three methods (N for each method is 405) are shown below:

AE	4.975
AJ	5.044
AEwkr	5.106

The deviations and mean scores obtained on the first, second, and third sittings are as follows:

First Sitting	5.09	(+40)
Second Sitting	5.005	(+ 2)
Third Sitting	5.022	(+ 9)

In Table II we present the *complete* Midas touch material to date. Included in this table are the data shown in Table I above, the original Midas touch material, and all the data collected under the original conditions (that is, exclusive of Experiment BA) subsequent to the publication of the first Midas paper.

Table II

Deviations of *first three runs* compared with deviations of *second three runs* called by naïve subjects, all data (August, 1941, to April, 1943).

Runs	*N (in decks)*	*Dev.*	*Mean*	*N*	$\frac{Dev.}{\sqrt{npq}}$	*P*
1	287	+102	5.35			
2	272	+ 35	5.13	816	3.17	.0008
3	257	+ 44	5.17			
4	242	+ 15	5.06			
5	232	— 35	4.85	699	.32	.37
6	225	+ 37	5.16			

Thus, only eight times in ten thousand similar experiments (each experiment extending over a period of twenty months) would such a result be expected on a chance basis; this result is, therefore, highly significant by standard statistical criteria.

Certain other analyses were made on the data. The following may be mentioned: (a) Salience (in respect of hits on the first card of each deck); preponderance of hits

in the first half of each deck; grouping of hits (runs of three, four, five, etc., successive hits); and abnormal score distributions (preponderance of extreme scores) as tested by Chi-square. Null results were obtained from these analyses.

An isolated case of striking scoring must, however, be noted. One subject, Miss L. L., calling her first deck of AEwkr (seventh deck of her first sitting), obtained a score of 17, the first fifteen cards of the deck being correctly guessed. An unbroken sequence of 15 hits at the beginning of a deck would be expected to occur by chance once in about 30,000,000,000 runs. It will be recalled that all decks used were arranged in accordance with random numbers.

Discussion of results: It is clear that no one method of the three possesses significant advantage over any other—at least under our conditions of experimentation. Further, the mean score obtained by all subjects under all conditions is without statistical significance. It will be noted, however, that the scoring level on Sitting One is higher than on either of the subsequent sittings, and that the average score of the first run of the first sitting was above chance (5.42). These results were predicted on the Midas hypothesis.

We believe that the most important contribution of Experiment BA lies in the fact that a definite type of result was *predicted and was obtained*—namely, a tendency to score high as a function of naïveté, with a subsequent decline to chance scoring. In other words, we suspect it is just possible that we have put the salt on the tail of that *rara avis,* the repeatable experiment in ESP.

In view of the one extreme score obtained by Miss L. L. (as noted above, the odds against correctly calling 15 cards in a row are astronomical), it may be of interest further to discuss the conditions attending this run, as well as the subsequent work of the subject. LD was experimenter throughout the series, but on the occasion of Miss L. L.'s first sitting it happened that ET was present in the room where LD was operating the machine and GM was in another room at the A.S.P.R. office. When the first five

or six guesses registered as correct, LD asked ET closely to observe the rest of the run. This he did. At the termination of the run under discussion GM's advice was sought on how to proceed. He suggested that there should be no change from the usual procedure as described above; that, in other words, Miss L. L. should continue her task of calling the remaining two AEwkr decks without interruption from us. After these decks had been called (yielding scores of 4 and 6) LD entered the room and, without telling the subject of her unusual score, engaged her in conversation. Miss L. L.'s introspections were as follows:

In the "17" run she had started out by looking around the room and certain objects there, which she associated with the five symbols, had successively caught her eye. For instance, when the coils of the radiator seemed to "attract" her she chose the wave; dry cell batteries on a near-by shelf indicated the circle; the corner of the table indicated the star; the square was symbolized by a chair-bottom, and "something she saw in the design of the rug" meant the cross. (Actually there is nothing in the design of the rug, which is of an Oriental type, that even remotely resembles the form of a cross.) Then, as the run progressed, this procedure began to seem too stereotyped, too "automatic," and another approach to the task took its place. She *felt* that she should call, say, a circle, and then she looked around the room until she found an object the outline of which would "justify" her impression. She further stated that after a while (whether towards the end of the "17" run or at the beginning of the next run could not be exactly ascertained) she "got bored with looking around the room and just tried to imagine what symbol Mrs. Dale was looking at, but thought it silly." She was not told at the end of this sitting, as were all the other subjects, what her exact scores were, nor was she paid the reward for her score of 17 until after the last sitting. Her total deviation for Sitting One was +14; Sitting Two, +8; Sitting Three, —4. When Experiment BA was completed, Miss L. L. was kind enough to come on two occasions to take part in further experimentation. The results obtained were of a chance nature.

Summary: An experiment was designed to facilitate the appearance of decline phenomena (Midas touch effects). 45 subjects called, during three sittings each, a total of 1215 decks of ESP cards, using three different methods of calling. Suggestive, but not statistically significant, decline effects were obtained, these effects being similar to those previously noted. When the results of the present experiment are pooled with the results published in the April issue of this JOURNAL, the P-value, based upon the positive deviation of the first three decks called by naïve subjects, is .0008. The Midas trend in the present data is less pronounced than in the previously reported material, but the total result remains statistically significant.

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Further Consideration of Multiple-Blocking and Unblocking in Normal Subjects

(Constituting a sequel to "Report of Investigations into an Obscure Function of the Subconscious Mind," published in the October, 1942, issue of this JOURNAL.)

HUGH WOODWORTH

[Upon publication of Mr. Woodworth's first paper, we were so impressed with the possibilities of his hypotheses that a repetition of the experiment was initiated at the Society. The results of this experiment are interesting and will be described immediately following Mr. Woodworth's paper. We continue to stand in friendly disagreement, however, with the author on a number of purely statistical points. We have indicated our views in footnotes and in a brief appendix. We shall, of course, grant the author an opportunity to reply.—*Ed.*]

Readers may remember that in "Report of Investigations into an Obscure Function of the Subconscious Mind" a detailed analysis was made of the score results of two subjects, each of whom called 60,000 ESP cards. The purpose of the study was not to present evidence of ESP itself, but rather to demonstrate a peculiar subconscious mechanism in subjects who manifested no ESP ability in the ordinary sense.

To recapitulate briefly, the purpose of the work was to substantiate the following hypotheses:

1. That extra-sensory awareness is a normal ability of the subconscious mind, i.e., that there continually impinges upon the subconscious minds of most (or all) people the very information for which the conscious mind is (at a given moment) seeking. And that this information arrives independently of the ordinary sense channels.

2. That the subconscious mind ordinarily experiences a distaste for such extra-sensory images and concepts impinging upon it, and rejects them, thereby preventing their access to the conscious mind. (This subconscious distaste is called the "blocking" or "rejecting" factor. It operates far below the conscious level.)

3. That the blocking or rejecting factor is variable and accounts for high, zero, and negative ESP results; i.e., that high ESP scoring results from a partial suspension of the blocking factor (or "unblocking"), that zero ESP scoring represents 100 percent rejection of the subconscious information, and that negative or below-chance scoring results from "mutiple-blocking" as explained in the original paper* (pp. 198 ff.).

Now, it has been shown that if a group of scores giving a chance *average,* give at the same time a *distribution* in which there is an excess of both high and low scores at the expense of mean scores, this distribution constitutes supporting evidence of the hypothesis. The scores of H. W. and R——, the two subjects studied in the main paper, gave over-all averages of 5.005 and 5.1075 respectively, the first clearly a chance average, and the second (C.R. = 2.6) indicative of some ESP that had to be taken account of in the analysis.

It remained to be seen, then, whether the *distribution* of scores making up these averages would show an excess of extreme scores at the expense of mean scores. Three types of distribution were investigated, and it was my opinion that the looked-for distortions indeed appeared.

It is here, however, that difficulty arises—the old difficulty of deciding whether a given distortion is a significant one, or can be attributed to the laws of chance. The data in the original paper, as the Editor points out, "are not treated by the currently accepted statistical procedures." This deficiency I now undertake to remedy. In addition I wish to submit the results of a third subject, F——, whose recent 60,000 trials constitute a block of work comparable to that done by R—— and by myself (H. W.).

F——'s experiments were conducted in precisely the same way as were the two other groups of 60,000 trials, but with the added precaution of independent checking of

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* Briefly, mutiple-blocking consists of rejection not only of an extra-sensory image impinging upon the subconscious mind, but also of a similar "normal" image, i.e., one that arrived in the mind through the ordinary sense channels.

results. F——'s over-all average is 4.989, satisfactorily non-significant of ESP ability.

It should be noted here that a subject who manifests ESP in the ordinary sense is not a suitable subject for the present study. We are looking for what is in effect a *flattening* of the normal curve (dotted line), with resulting upward thrusts at either end, as shown in Fig. 1. This distortion, by its very nature, can scarcely result from the scoring of any subject whose over-all average is not extremely close to chance. For whenever an ESP factor is at work, the curve of score distribution must show a displacement to the right—a displacement that almost inevitably precludes persistent outcropping above the normal curve of any low-score totals. Fig. 2, showing the graph of a hypothetical slightly clairvoyant subject, illustrates the point.

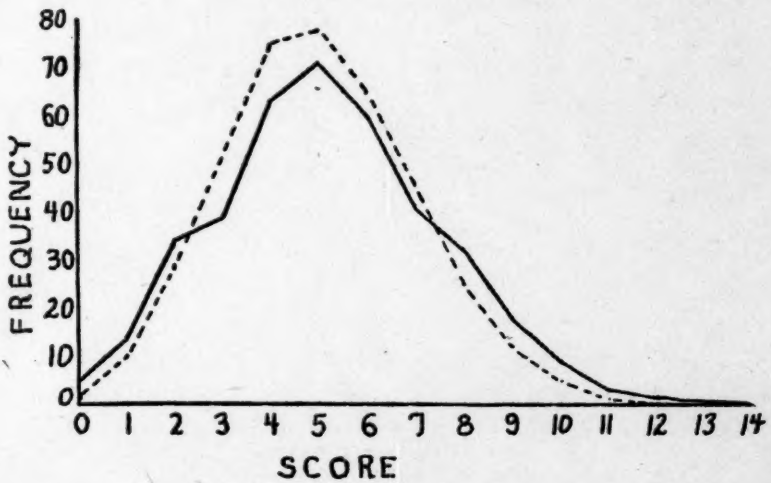

FIG. 1.
Typical multiple-blocking and unblocking graph (Hypothetical).

It is true that the flattening effect may appear even in the scores of a subject manifesting ESP, but it is seldom great enough to overcome the displacement to the right sufficiently to break through the normal curve at its left end. And when it rarely does, the break-through is trivial and unimpressive.

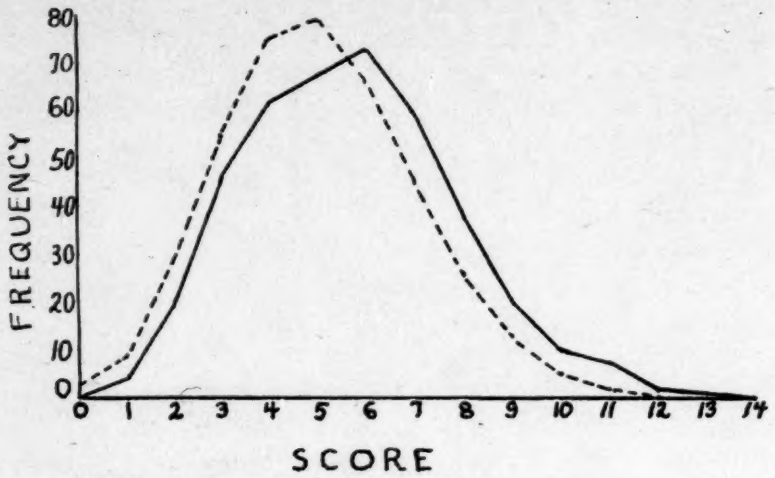

FIG. 2.
Typical ESP distortion (Hypothetical).

Direct ESP, therefore, is a factor in scoring that can only conceal, minimize, or preclude the evidence we are looking for. It follows that R——'s scoring, for our purposes, must be assessed with reservations. The fortunate thing is that with the completion of F——'s work there are now two lots of scores (F——'s and my own) which average extremely close to chance.

Three types of score distribution are investigated:

First there are the individual scores themselves. Each individual score derives from 25 trials, or a single run through the ESP deck. 60,000 trials produce 2400 such scores, ranging (in the results under consideration) from 0 to 14. The expected average score is 5.

Second. In the actual experiments there was a tendency for individual scores to be obtained in groups or "lines" of ten at one sitting. As the sequence of all scores was preserved when they were listed, it was thought that the "line-totals" might reveal incidence of multiple-blocking or unblocking. Accordingly the distribution of such line-totals was studied. The expected average is 50, and the actual totals range from 29 to 74.

Third. In order to discern any evidence of mutiple-blocking or unblocking over periods of time shorter than that required to obtain ten individual scores, the lines-of-ten-scores were halved and the distribution of the resulting "quadrant line-totals" was investigated. The expected average here is 25, and the totals were found to range from 10 to 44.

1. *Distribution of Individual Scores*

Table I gives the mathematical probabilities for each score, and the results obtained by each of the three subjects. It will be seen that in the cases of both H. W. and F—— excesses of both high and low scores appear. F——'s scores of 0, 1, and 2 are 40.2 in excess of expectation, give a C.R.* of 2.76, and a P-value of .003.† F——'s scores of 10 and over are 15.4 in excess of expectation, give a C.R. of 2.41, and a P-value of .008.

My own low and high scores are also in excess of expectation, but to a lesser extent. My 0's, 1's, and 2's give a C.R. of 1.38 and a P-value of .08; my 10's and over give a C.R. of 1.63 and a P-value of .05. From a statistical point of view these values are not significant, but perhaps they may borrow some significance from the fact that they parallel the trend of F——'s results.

If the excesses at either end of the distribution are combined, they give a C.R. considerably greater than that of either end alone. F——'s low and high scores combined give a C.R. of 3.55 and a P-value of .0002. My own combined excesses give a C.R. of 1.95 and a P-value of .026. Table II summarizes all these results.

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* I have calculated the standard deviation from the formula S.D. $= \sqrt{npq}$ or S.D. (in this case) $= \sqrt{2400 \times \frac{235.8}{2400} \times \frac{2164.2}{2400}}$ or 14.58. The actual deviation divided by the S.D. gives the critical ratio (C.R.), from which a P-value can be found.

† The P-value indicates the probability that a given result would occur in purely chance operations. It is conveniently thought of as a fraction, i.e., $.003 = \frac{3}{1000}$, which means that the result under consideration could be expected to occur, in chance operations, 3 times in 1000 repetitions of the entire experiment. Some of the P-values given in this paper, being interpolated, are approximate.

Table I

Distribution of 2400 individual scores.

F——, H. W., and R——.

Score	Expectation	F——		H. W.		R——	
0	9.1	18	dev. +40.2	9	dev. +20.2	6	dev. —68.0
1	56.7	70		61		54	
2	170.0	188		186		165	
3	325.8	317		315		296	
4	448.0	413		466		439	
5	470.4	455		436		452	
6	392.0	385		380		442	
7	266.1	288		259		259	
8	149.6	146		176		156	dev. +25.1
9	70.7	63		60		75	
10	28.3	43	dev. +15.4	34	dev. +10.4	38	
11	9.6	12		11		14	
12	2.8	1		6		4	
13	.7	—		—		—	
14	.2	1		1		—	
	2400.0	2400		2400		2400	

R——'s distribution, as might be expected from the over-all average of 5.1075 (C.R. = 2.6), reveals ESP. The scores from 0 to 5 inclusive are all less than expectation, and those of 8 and over are in excess. This distortion, as has been explained, can generally be counted on to preclude

Table II

C.R.'s and P-Values deriving from Table I.

(Groups of Scores)

	F——				H. W.			
	Dev.	S.D.	C.R.	P	Dev.	S.D.	C.R.	P
Scores of 2 and under	+40.2	14.58	2.76	.003	+20.2	14.58	1.38	.084
Scores of 10 and over	+15.4	6.39	2.41	.008	+10.4	6.39	1.63	.052
Above scores combined	+55.6	15.66	3.55	.0002	+30.6	15.66	1.95	.026

the appearance of that balanced distortion (F——'s and H. W.'s above) which constitutes evidence of multiple-blocking and unblocking. (It is worth noting, however, that the balanced distortion can and sometimes does exist *within* the ESP distortion. But its detection then requires methods we cannot consider here.)

R——'s scores of 8 and over, it might be mentioned, give a C.R. of 2.2 (P = .0139), but this is evidence only of ESP.

I have applied the Chi-square method of appraisal to the distributions of F—— and H. W. in Table I, with the following results:

F——	$\chi^2 = 27.8$	$n' = 12$	$P = .003$
H. W.	$\chi^2 = 15.0$	$n' = 12$	$P = .182$

When the Chi-square of a given distribution turns out to be so low as to give a P-value of .182 or thereabouts, it is the practice among some statisticians to dismiss the entire experiment as non-significant. Such a practice, however, is not always justifiable, for although the Chi-square method is reliable nine times out of ten, it can easily leave undetected results of high significance. Table III, a *hypothetical* distribution to which the Chi-square method has been applied, illustrates the point. Although the P-value of the distribution as a whole is .18, the 0-scores are greatly in excess of expectation, and provide a deviation that, worked out separately, gives a P-value of .0002.

Another weakness of the Chi-square method is that it often necessitates the grouping of extreme scores—scores which may be significant separately but not *en bloc;* and still another weakness is that the method takes no account of any existing pattern in the deviations—a pattern such as mine in Table I, comprising a consistent excess of both high and low scores. Such patterns can constitute highly suggestive clues to ESP functioning, especially when they are found to occur in a number of subjects.

Table IV shows a number of C.R.'s and P-values deriving from individual scores in Table I. For the most part,

Table III

The Chi-Square method applied to a hypothetical distribution.

(Illustrating one of the defects of the method)

Score	Expected	Obtained	Difference	Difference²	Difference²/Expected
0	9.1	20	10.9	118.81	13.05
1	56.7	55	1.7	2.89	.05
2	170.0	171	1.0	1.00	.01
3	325.8	323	2.8	7.84	.02
4	448.0	452	8.0	64.00	.14
5	470.4	465	4.6	21.16	.05
6	392.0	385	7.0	49.00	.12
7	266.1	276	10.0	100.00	.38
8	149.6	145	5.6	31.36	.21
9	70.7	73	2.3	5.29	.08
10	28.3	25	3.3	10.89	.38
11	9.6	7			
12	2.8	2			
13	.7 } 13.3	1 } 10	3.3	10.89	.82
14	.2	0			
					15.31

$\chi^2 = 15.31$ $\quad n' = 12$ $\quad P = .18$

When the 0's are worked out separately, however, the standard deviation is $\sqrt{2400 \times \frac{9.1}{2400} \times \frac{2390.9}{2400}}$ or 3.01, the critical ratio is 3.62, and P = .0002.

Table IV

C.R.'s and P-Values deriving from Table I.

(Individual Scores)

Score	Subject or Subjects	Dev.	S.D.	C.R.	P
0's	F——	8.9	3.08	2.95	.0016
1's	F——	13.3	7.44	1.79	.038
2's	F—— & H. W. combined	34.0	17.75	1.91	.028
5's	H. W.	34.4	19.37	1.77	.039
5's	F—— & H. W. combined	49.8	27.46	1.81	.035
8's	H. W.	26.4	11.88	2.20	.014
10's	F——	14.7	5.28	2.78	.0028
*14's	F—— & H. W. combined	1.7	.547	3.11	.001

*The number of 14 scores mathematically expected in 2400 runs is actually .15, not .2 as shown in Table I which gives only one decimal place.

the C.R.'s are rather low, especially mine, as the Chi-square P-value of my whole distribution would lead us to expect. The fact that a Chi-square result is not, however, always confirmed by an appraisal of its parts, is evident later, in the investigation of quadrant line-totals.

The P-values given in Table IV and elsewhere are taken directly from a Normal Curve of Error, and have not been doubled as perhaps, in some cases, they should be. Whether they should be doubled or not is largely a matter of interpretation, so I am leaving them as they are.

A noteworthy thing about my own score distribution is that the high and low score excesses are confined almost entirely to a certain section of the work. When the six blocks, or sections, of 10,000 trials making up the 60,000 total are considered separately, the third such section from the beginning shows the multiple-blocking and unblocking

Table V

Chi-Square of H. W.'s third block of 10,000 trials.

Score	Expected		Obtained		Difference	Difference^2	Difference^2 / Expected
0	1.5		3				
					10.1	102.01	9.36
1	9.4		18				
2	28.3		38		9.7	94.09	3.32
3	54.3		33		21.3	453.69	8.36
4	74.7		86		11.3	127.69	1.71
5	78.6		73		5.6	31.36	.40
6	65.3		53		12.3	151.29	2.32
7	44.3		38		6.3	39.69	.90
8	24.9		39		14.1	198.88	7.98
9	11.8		11		.8	.64	.05
10	4.7		6				
11	1.6		1				
12	.5	6.9	0	8	1.1	1.21	.18
13	.1		0				
14	.0		1				
	400.0		400				34.58

$\chi = 34.58$ $\quad n' = 10$ $\quad P = .00006$ (approx.)

distortion with marked effect. The distribution of this section of the data is graphed in Fig. 11, page 214, of the original report. Its Chi-square, shown here in Table V, gives a P-value of .00006, exceptionally low.

2. *Distribution of Line-Totals*

The line-total extremes are shown in Table VI. F——'s extreme totals are very close to expectation. R——'s work shows a deficiency of low totals and an excess of high ones, again revealing the ESP factor. My own high totals are slightly in excess of expectation, and my low totals considerably in excess—perhaps indicating that over a period of time necessary to obtain ten ordinary scores, the multiple-blocking factor has sometimes showed itself. The C.R. of my low totals is 2.31, giving a P-value of .01.

Table VI

Extreme line-totals and column-totals per 60,000 trials.

Extreme Line-Totals

	40 and under	60 and over	combined
Expected	15.28	16.52	31.80
F——	13	19	32
H. W.	24	18	42
R——	9	25	34

Extreme Column-Totals

	40 and under	60 and over	combined
Expected	15.28	16.52	31.80
F——	19	18	37
H. W.	24	18	42
R——	12	30	42

When the lines-of-ten-scores are added vertically in groups of ten, "column-totals" are obtained. Any significant variations in these indicate consistent fluctuations of ES factors in the course of the experiments, as explained in the first paper. It will be seen that my column-totals of 40 and under are also in excess, giving the same C.R. of 2.31 and P-value of .01.

When my low line-totals and low column-totals are considered in conjunction, however, the C.R. jumps to 3.23 and the P-value to .0007. It is true that low line-totals and low column-totals indicate somewhat different things, but it is permissible to add them together in our search for statistical significance.*

3. *Distribution of Quadrant Line-Totals*

The distributions of quadrant line-totals for all three subjects are shown in Table VII. They provide a good illustration of the limitations of the Chi-square method, for although the Chi-square P-values are entirely non-significant, it can be shown that the distributions nevertheless possess distortions of considerable significance.

Table VII shows that the flattening effect appears most strongly in my own data. My quadrant totals of 19 and under are in excess; likewise those of 30 and over. Combined, they give a C.R. of 3.03 and a P-value of .001.

R——'s data show the usual displacement to the right, with an excess of high scores and a deficiency of low ones. It may be worth noting, however, that the *combined* high and low scores are in excess, perhaps indicative of the flattening effect *within* the characteristic ESP displacement. This excess, however, is not statistically significant.

F——'s quadrant line-totals are almost exactly equal to expectation.

The examination of totals above and below certain points can reveal multiple-blocking and unblocking only when the evidence is gross. Often, as might be expected, the effects of blocking and unblocking are devious, complicated, or subtle. Sometimes the evidence is seen more easily in the flattening of the center of the graph rather than in the bulging at the ends. In all three sets of data, for example, a significant flattening† is seen in the totals of the 24-scores, which combined yield a C.R. of 2.5 and a P-value of .007.

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* The degree to which line-totals and column-totals are interdependent has not been computed, and the P-value given is probably considerably too low.—*Ed.*

† Most marked in F——'s, where the extreme scores show no significance.

Table VII

Quadrant Line-totals.

Score	Expected	F——	H. W.	R——
7	.002	0	0	0
8	.007	0	0	0
9	.021	0	0	0
10	.062	0	1	0
11	.163	0	0	0
12	.388	0	0	0
13	.843	2	1	0
14	1.686	1	1	1
15	3.119	2	4	5
16	5.361	7	11	2
17	8.593	9	10	10
18	12.889	13	17	12
19	18.147	17	21	15
20	24.048	19	19	16
21	30.056	43	25	28
22	35.529	37	37	43
23	39.768	44	39	37
24	42.253	30	34	37
25	42.667	38	45	35
26	41.026	40	34	46
27	37.617	41	35	35
28	32.922	35	31	38
29	27.523	24	25	24
30	22.018	24	26	26
31	16.866	17	23	24
32	12.387	13	10	19
33	8.726	11	13	6
34	5.903	4	4	5
35	3.840	7	6	5
36	2.399	1	4	4
37	1.443	1	2	0
38	.835	0	1	3
39	.466	0	0	2
40	.250	0	1	0
41	.130	0	0	0
42	.065	0	0	1
43	.031	0	0	0
44	.014	0	0	1
45	.006	0	0	0
46	.003	0	0	0
47	.001	0	0	0
	480.073	480	480	480

One curious characteristic of multiple-blocking and unblocking is that their effect on a graph is never *simple,* as might be thought (see Fig. 10 in the original paper), but *compound.* Instead of a mere flattening at the center and a bulging at the ends, the typical graph is found to possess certain more or less symmetrical (and often significant) dips and peaks (Figs. 11 and 12, original paper). And such dips and peaks, I think, are really to be expected on the grounds of the hypothesis. If multiple-blocking exists, its effect would be to "throw" a certain percentage of all scores to the left of where they might be otherwise expected to occur. Unblocking, contrarily, would throw others to the right. This might account, in the first place, for the peak which often occurs at the center of a graph.

Other effects, however, would be included, for the following reasons:

It must be borne in mind, first, that multiple-blocking and unblocking have their incidence during a certain undetermined *percentage of the time* occupied by the experiment; and second, that without these factors, the subject's scores would approximate a normal graph. It follows from this that although the percentage of distortion affecting each score is the same, the *amount* of the distortion will be greatest in those scores that normally occur most frequently, i.e., in those represented by the central section of the normal graph.

Here the matter becomes complicated. Every point on the graph must be thought of as a center from which two small dislocations are occurring, one to the right and one to the left. The confusing thing is that the dislocations are greatest towards the top of the graph and least towards its ends.

Probably this complex shifting is best thought of as a series of small waves occurring along the whole line of the graph, being greatest at the top, and always moving to both right and left from any given point. Granted this reasoning, it follows that the waves will appear with greatest amplitude at the top of the graph, and that down its sides might occur points where waves, reinforcing each other,

would form nodes or crests, with subsequent dips between them.* Fig. 12 in the original paper seems to be a particularly good illustration of these possible effects.

Still further complications, however, exist. The incidence of multiple-blocking and unblocking no doubt varies somewhat among different "normal" subjects, and in the same subject at different moments. A score of 7, blocked, might become a 6 at one time, and a 5 at another. Taking all this into consideration, it can hardly be expected that the graphs of the scores of any two normal subjects will show much similarity in their dips and peaks. And indeed, such similarity is rare. The curious thing is that it should exist at all—for it does appear to exist to some degree, and with some significance, in the three subjects under consideration. The similarity of the 24's in the quadrant line-totals has been already mentioned; the 20's likewise are similar, each being below expectation; combined, their C.R. is 2.5, giving a P-value of .007. If the 20-scores and the 24-scores of all three subjects are taken in conjunction, the C.R. jumps to 3.3 and the P-value to .0005.

The three subjects likewise have in common a dip at 29, a crest at 30, and a smaller crest at 17—although these are

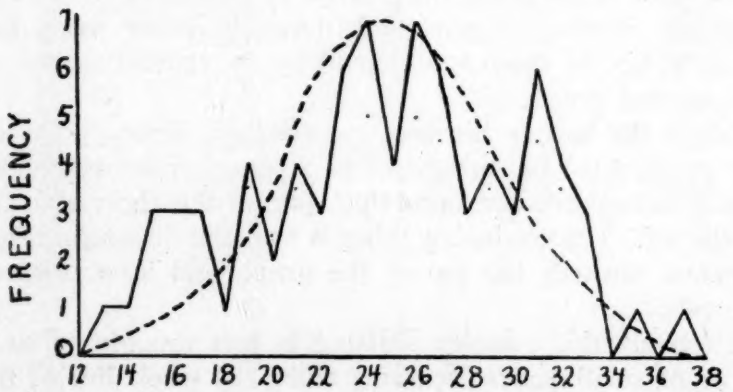

FIG. 3.
Quadrant line-total distribution of H. W.'s third block of ten thousand trials.

* The nodes and dips should be compared with the actual irregularities in more or less normal curves where psychic factors of the type postulated are known to be absent.—*Ed.*

not significant. It is rather unexpected that R——'s graph, having its own ESP distortion, should show these similarities with the others.

Most remarkable of all multiple-blocking and unblocking graphs is that resulting from the quadrant line-totals of my peculiar third block of 10,000 trials. It is shown in Fig. 3. Counting as extreme scores those of 17 and under and 31 and over, the C.R. is 4.3, which has a P-value too small to be included in my tables.*

Appendix

Editorial Comment on Statistical Procedures

In our judgment there is no great purpose in applying the critical ratio method to groups of high and low scores when the standard which determines what is "high" or "low" shifts arbitrarily. This procedure naturally encourages the habit of taking 3, or 2, or some other figure as a standard of "lowness," and 8, or 9, or some other figure as a standard of "highness," depending on which gives the most impressive result. In our opinion it would be sounder procedure to stick to one definition throughout a series of experiments; e.g., on the basis of empirical findings (suggesting where the effect appears) we might set 8 or better as "high," 2 or lower as "low," and if the result is significant, postulate, as an hypothesis to be tested, that in subsequent work the highs and the lows *as defined* will be significantly in excess of chance. But if they are, and if the effect is reasonably consistent, as the author maintains, the Chi-square method, despite the considerations he urges, will probably reveal it. If need be, the critical ratio method may be used, but in terms of standards fixed in advance, not adjusted after the event.

* According to Pearson's Tables, P = .000008.—*Ed.*

Note on an Attempt to Repeat the Woodworth Results

When, in the summer of 1942, we published Hugh Woodworth's first paper, "Report of Investigations into an Obscure Function of the Subconscious Mind," it was decided to have a subject call 60,000 ESP cards; the results of these calls could then be compared with the results of Mr. Woodworth's subjects and independent evidence of multiple-blocking and unblocking might be obtained. Being available at all times, the writer (LD) was chosen to act as subject.

Material: Standard ESP cards were used. A "master-deck" containing 100 of each symbol was thoroughly shuffled before each sitting and dealt out into 20 decks, these decks then being inclosed in 20 individual numbered boxes. There was not, therefore, a fixed number of each symbol in the decks called by the subject. Miss Wellman, the Executive Secretary, prepared the stimulus material and later recorded the card order of each deck on standard record sheets.

Procedure: Each day (from August 31, 1942 to March 2, 1943, with "days off" during vacations, etc.) Miss Wellman presented 20 boxes to LD who then wrote down her guesses as to the order of the cards in each box. A DT (down-through) procedure thus was used (in contra-distinction to Mr. Woodworth's use of the open-matching method). On some days LD's calls were unwitnessed; on other occasions she made her calls in the presence of anyone who happened to be in the office. On completing the day's task, she turned over her record sheets to Miss Wellman, who then opened the boxes and recorded the card order, later comparing this record with LD's. Scores for the day were entered in a special notebook. At the end of the entire experiment, two Hunter College students again compared all record sheets and rechecked for errors in scoring of hits. It is of interest to note that all errors found in the recheck were failures to observe correspondences between target and calls; that is, failure to check hits.

Results: 2400 decks were called (20 decks a day for 120 days) and the average number of hits per deck was 4.95. This over-all result is without statistical significance (C.R., —1.19). Obviously LD displayed no ESP ability in the ordinary sense. According to Mr. Woodworth's hypothesis, a comparison of obtained results with results expected by chance should reveal that extreme scores, high and low, are in excess of expectation—at the expense of scores near the mean (4's, 5's and 6's). (See Fig. 10, p. 213, of Mr. Woodworth's original report, in the October, 1942, issue of this JOURNAL.) The excess of extreme high and low scores is attributed by him to multiple-blocking and unblocking.

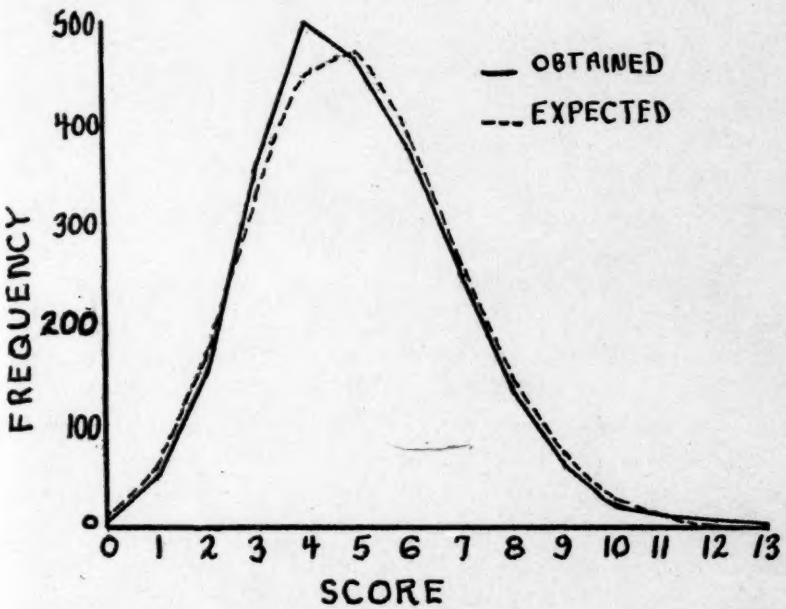

FIG. 1.
Distribution of scores, 2400 decks.

Fig. 1 (of this paper) shows the relation between scores expected by chance and the scores obtained in this experiment. The value of Chi-square is 25.3 and P is .01. It is interesting to compare this figure with Mr. Woodworth's Fig. 10, previously noted, and with Fig. 1 appearing in his article, p. 121, this issue. It will be noted that in Mr.

Woodworth's Fig. 1 scores from 3 to 7 inclusive are below expectation, all other scores being in excess of chance. In LD's data, however, the deviations from chance expectation do not take the same form. If LD's results were in complete agreement with Mr. Woodworth's hypothesis, all scores except those from 3 to 7 inclusive would be above chance expectation. Actually, though, the only scores whose frequencies *exceed* chance expectation are the 3's and 4's and the 12's and 13's. It must especially be noted that scores of 0, 1, and 2 and 8, 9, and 10 are *below* chance expectation. Also, the greatest single contribution to the Chi-square is found in the departure from chance expectation of scores of 4. According to Mr. Woodworth's Fig. 1, scores of 4 should be noticeably less than expected; quite the contrary is the case in LD's data. It is entirely possible, however, that individual differences exist in the locus and extent of the multiple-blocking effect; this is as yet undetermined. As for a possible unblocking effect, it should be pointed out that LD's excess of scores of 12 and 13 contributes nine points to the total Chi-square.

Whether or not these data may be considered as affording evidence in support of the hypothesis of multiple-blocking and unblocking, the fact remains that the departure from chance expectation obtained (in regard to score distribution) approaches statistical significance. It may be remarked that inspection of much of LD's other data reveals the same tendency; namely, to obtain an excess of scores of 4 and of 11 or better. Also, it may be mentioned that the average score of the *first deck* called each day was 5.25. The average score on all remaining decks was 4.93. While this difference is not statistically significant, it is suggestive of the operation of the Midas touch, discussed elsewhere in this issue.

Summary: An experiment was undertaken the purpose of which was to obtain evidence of multiple-blocking and unblocking, as defined by Mr. Woodworth. Frequency distributions comparing obtained and expected results showed effects different from those predicted by the hypothesis.

Either (a) the Woodworth hypothesis requires further clarification and better support or (b) the multiple-blocking and unblocking effect in LD's case is dissimilar from these effects as they appear in the calling of Mr. Woodworth and his subjects. Further data are required to settle the point. It seems to us that any hypothesis attempting to clarify the problem of below-chance scoring is of real importance and it is to be hoped that other interested persons will repeat Mr. Woodworth's experiment.

L. A. DALE

In Memory of Dr. Gustav Pagenstecher

LYDIA W. ALLISON

A letter from Mrs. Gustav Pagenstecher announces the death of her husband, Dr. Pagenstecher, on December 26th, 1942, when he was approaching the eighty-eighth anniversary of his birth. The letter also brings the news of the death of Dr. Pagenstecher's remarkable non-professional medium, Mme. Reyes Zierold, known as Señora Maria Reyes de Z., which occurred last September during a visit to her daughter and grandchildren.

Older members of the Society will recall the widespread interest in the striking evidential statements of Señora de Z. when reports of the experiments were published by Dr. Pagenstecher and by Dr. Walter Franklin Prince more than twenty years ago. Today these reports remain unsurpassed in the special field of psychometry. When in hypnotic trance, Señora de Z. was able to perceive the mental state of her physician and also to give relevant facts beyond the reach of chance concerning a great variety of unknown objects placed in her fingers.

After Dr. Pagenstecher accidentally discovered the apparently supernormal powers of Señora de Z. he began experiments with her in 1919. The results were so surprising that he brought them to the attention of the leading Mexican medical society, which appointed a commission to verify them. The members of the commission witnessed a number of experiments and upheld Dr. Pagenstecher's claims that the observed phenomena were of a supernormal nature.

Dr. Prince's acquaintance with Dr. Pagenstecher had begun with a correspondence and continued until the weight of the accumulating evidence induced him to go to Mexico in 1921, in order to investigate Señora de Z. independently, and also to learn all possible facts concerning the earlier work. The two investigators became great friends and colleagues.

Dr. Prince had seven experimental sittings with Señora de Z. under his own conditions. While the quality of his results did not measure up to the standard of those obtained by Dr. Pagenstecher, Dr. Prince noted that hardly any details were incorrect and he was fully satisfied that the "hits" taken together seemed quite inexplicable on a chance theory. As Dr. Pagenstecher's own manuscript was placed at the disposal of Dr. Prince and edited and exhaustively discussed by him for the English reading public, I find it difficult to speak of one man without including the other, although these lines are written primarily in memory of Dr. Pagenstecher.

Dr. Pagenstecher was descended from a family of scholars and officials and was a graduate of the University of Leipzig. He was a physician of distinction in the City of Mexico, where he had long made his home. He was highly regarded as a public speaker and had twice been invited to deliver addresses on great public occasions in the presence of the President and his cabinet.

Dr. Pagenstecher's reactions to his fortuitous discovery of the powers of Señora de Z. are best described by quoting from Dr. Prince's report (see *Proceedings,* Vol. XV, 1921, p. 190).

"Dr. Pagenstecher had been a materialist for forty years. He was not looking for any change in his philosophy, nor did he have any expectation of strange phenomena when he began to hypnotize Señora de Z. for therapeutic purposes. She herself was not aware that she possessed any peculiar powers. But when she began to manifest knowledge of existing facts supposed to be out of the reach of her normal senses, the hypnotizer, actuated by that curiosity, or interest in matters yet obscure, which is the impelling force of all scientific discovery, began to experiment deliberately . . .

"The letters written to me by Dr. Pagenstecher testify to the strenuosity with which he endeavored to maintain his strict materialistic principles; and none of them, up to the time of my visit to Mexico, distinctly announced relinquishment of these, but it was read between the lines that

he was hard pushed, and even forced over the boundary line. My arrival found him convinced that, as Huxley admitted, there is something in the universe transcending matter and force, and he seemed half amused to see himself in a different camp from that which he had occupied for forty years, led there by conclusions from the facts observed which he did not feel that he could logically and honestly evade."

In his foreword to Dr. Pagenstecher's report Dr. Prince described Señora de Z. as a woman of unusual intelligence, better educated than the average girl in Mexico and the daughter of a former divisional General of the Federal Army, and Governor of the State of Michoacan. Owing to her discovery of fraud during a brief contact with spiritualism as a girl, Señora de Z. had always felt a repugnance for it. For many years her attention had been engrossed by the care of a large family. The phenomena she exhibited were as great a surprise to her as they were to Dr. Pagenstecher, who began hypnotic treatment solely as a means of overcoming insomnia.

Dr. Prince received the impression that Señora de Z., whom he studied in every possible manner, both in the sittings and in her home, as well as on social occasions, regarded her gift in a matter-of-fact fashion. While she was interested in her phenomena, she would not have cared for the experiments, had it not been for her gratitude to the man who by his professional skill had probably saved her life.

The value of Dr. Pagenstecher's contribution to psychical research can hardly be overestimated. "It was evident," and again I quote Dr. Prince, "that great moral courage had been manifested in going before a medical society at least mostly composed of skeptics and demanding that there be placed on record his affirmation that 'in order to hear, to see, to smell, and to taste it is not absolutely necessary to have ears, eyes, tongue and nose,' and that a commission be appointed to test his statements by personally participating in experiments with Señora de Z. Such a claim seriously jeopardized his professional standing and his practice itself,

unless he could substantiate his claims. It was certain that, in spite of their general skepticism of such matters, the commission, including some of the leading physicians of Mexico, did witness to the success of the strange experiments in which they shared." (*Op. cit.*, pp. 191-192.)

It may be added that some leading psychical researchers feel that Dr. Prince's most important studies were made in the field of psychometry and chief among these was his investigation of Señora de Z. Dr. Pagenstecher granted Dr. Prince full liberty to introduce any feature he wished into the experiments, instantly produced any desired original or collateral document and cheerfully answered every question. This attitude on the part of Dr. Pagenstecher enabled Dr. Prince to bring the full powers of his intensely critical mind to the study of the case. It was no small tribute to his colleague that Dr. Prince felt, both while in Mexico and after his return to New York, that Dr. Pagenstecher had understated the facts.

In 1935 nothing had been heard from Dr. Pagenstecher for a long time. Then one day a letter came for Dr. Prince, more than a year after the latter's death in August, 1934. It began, "These lines will be for you almost the greeting of a dead man who has resurged only to give you a great pleasure." The letter was concerned with predictions made fourteen years earlier by Señora de Z. while in the deepest state of cataleptic trance. On November 24, 1921, Dr. Pagenstecher had sent a letter to Dr. Prince enclosing the predictions in three separate envelopes, each sealed with his personal stamp. They were to be held for safe-keeping and opened at Dr. Pagenstecher's death, or earlier if he so authorized. It appeared to the doctor that some of the predictions were beginning to be fulfilled.

Dr. Pagenstecher was deeply distressed when the news of Dr. Prince's untimely death reached him. It was a "real blow." In his reply he wondered what Dr. Prince might have thought of his silence of years. He explained the reasons. He had not wished to make Dr. Prince unhappy by telling him that after the first World War the small fortune he had invested in Germany had been entirely lost

and that he had been obliged to give up his beloved study of psychometry and the reading of psychical research literature to devote himself again to hard work in his practice. There was no hint of complaint.

At about the same time, Dr. Pagenstecher sent me a delightful and reassuring photograph, taken some months earlier. It was an informal picture of himself and his wife in their home made on the occasion of his eightieth birthday. Dr. Pagenstecher still appeared to be a man at the height of his physical and intellectual powers, giving no indication that he had reached a venerable age.

It must have required character and courage for a man in his declining years to begin the struggle for existence anew. But in the friendly correspondence that continued, the general tone and enthusiasm of Dr. Pagenstecher's letters were ample evidence that his spirit was undaunted. The revolutionary experience with Mme. Zierold, when he was well past sixty, had totally changed his sense of values and his outlook on life.

Cases

I

We are indebted to Mrs. Hugh Woodworth, of Nanaimo, British Columbia, for the interesting dream to follow. Mr. Woodworth, who corroborates his wife's account, is known to readers of this JOURNAL for a paper which appeared in the October, 1942, issue—"Report of Investigations into an Obscure Function of the Subconscious Mind." A sequel to this paper appears in this issue. Both Mr. and Mrs. Woodworth have had other spontaneous experiences of a parapsychological nature. Mrs. Woodworth wrote to us under date of May 10, 1943:

I am glad to give you a fuller account of a dream of mine that Mr. Woodworth has already mentioned to you.

The dream was in my mind on waking on the morning of Wednesday, November 11, 1942, and I told Mr. Woodworth about it before either of us got up. I dreamed that the postman had brought two parcels and some letters. One parcel was flattish, suggesting to me a card table folded up—not simply the legs folded, but the top as well. Its dimensions as I dreamed them would be about 12" by 7" by 1½". The flatness was the parcel's most noticeable feature, particularly as I dreamed that it and the letters had been slipped under the front door—although the parcel was not really thin enough to make this reasonably possible. On opening it, I found the contents to be white and flat, but on waking I could remember no other details, or whether, indeed, it turned out to be a table.

When I told Mr. Woodworth about the dream, he expressed surprise that a table should be folded, even in a dream, and in questioning me about it asked, "Like a tablecloth?" To this I assented.

All this was about 10 A.M. as November 11th was a holiday and we had slept late. When we finished discussing the dream, Mr. Woodworth got up, and called to me from the kitchen that there was some mail. This surprised us both, for three reasons: the fact that any mail should have been delivered on a holiday, the fact that it should have come as early as 10 A.M. (since mail *never* had been delivered earlier than 3 P.M.), and the fact that it should have been left at the back door, as it was. (The postman always comes to the front door unless there is a parcel and no one is at home to receive it. In this case he leaves it at the back door for safety's sake.)

Mr. Woodworth brought the mail into the bedroom; there was a letter for him and a parcel for me. (Both of which, incidentally,

had been outside the back door, not slipped under it.) My parcel was very nearly the same size as one of the ones I had dreamed of; it was an inch or two wider, and somewhat flatter—not being a box at all, but a cardboard folder. I opened it and found that it contained a white tablecloth four feet square. It was a totally unexpected gift from a friend in Vancouver. Neither of us had any reason to expect a gift at this time, still less a tablecloth. My birthday was not for two and a half months, Mr. Woodworth's not for four months, and our anniversary was five months away.

FREDA WOODWORTH

Mr. Woodworth inclosed a statement concerning the events recounted above, also dated May 10, 1943:

. . . I am glad to confirm briefly those details of the dream that I knew of at the time. We slept late on November the 11th, waking about 10 A.M. I no longer remember which of us woke first, but I should say we both awoke about the same time. Mrs. Woodworth told me she had dreamed the postman brought a parcel for her containing a table "folded up." I thought this an odd dream and questioned her about it. I said, "You mean the legs were folded?" and she said, "Yes, but the top was folded too." I found this rather incredible, and after a few more remarks asked if it were folded both ways like a tablecloth, to which she agreed.

She did not mention the dimensions of the parcel when she first told me about it, and indeed, I suppose she could hardly have considered these worth mentioning at the time. She did not mention the similarity in size even when I brought the parcel in, but spoke of it somewhat later. The parcel contained a white tablecloth, almost exactly four feet square. As Mrs. Woodworth has explained, it was a totally unexpected gift.

HUGH WOODWORTH

II

The percipient in the following case, which seems to afford evidence for survival, is a Member of the Society, Mildred C. Harris, of Philadelphia, Pennsylvania. Miss Harris, who prefers to use her maiden name in this connection, incloses with her original account corroboratory statements from her husband and from a friend, Mrs. Jean Galer, also a Member of the Society. The real names of the other persons concerned (pseudonyms have been used for obvious reasons) are on file with us. Miss Harris told us of the incident under discussion three or four days after

it occurred. We asked her to let us have a written statement and this she sent us under date of April 20, 1943:

For the past six or eight months, as you know, I have been having various experiences of a psychical nature. From time to time I receive strong impressions of matters normally unknown to me and, upon investigation, these impressions prove to be accurate. The incident you have asked me to record is one such.

On Thursday, April 8, 1943, Mr. *Hoyt** died in Philadelphia at approximately twelve o'clock noon. I learned of his death through normal channels. I must make it clear that, although I am an intimate friend of Mr. *Hoyt's* first wife (they were divorced more than fifteen years ago and Mr. *Hoyt* had married again), I had never, to the best of my knowledge, seen Mr. *Hoyt,* nor had his physical characteristics and appearance ever been described to me.

On the night of April 8th, that of the day on which Mr. *Hoyt* died, before dropping off to sleep about midnight, I mentally inquired if he were all right. At once I grew aware of an individual about medium height, a bit paunchy in girth, round-faced, with piercing eyes.

"Tell *Helen,* tell *Helen!*" he appeared to be saying, mentioning the given name of my friend, his first wife.

"What would you like me to tell her?" I inquired.

"Home! Home!"

"Do you mean that you are at home?"

"No, no! The high mahogany chest of drawers . . . has brass handles or hardware . . ."

A mental view of such a piece of furniture was so clearly impressed upon my consciousness that I could have drawn a picture of it in detail. Then Mr. *Hoyt* seemed to continue:

"The high mahogany chest with brass hardware . . . in the drawer where my handkerchiefs are . . . a black wallet . . . folder . . . tell *Helen!*"

Here I had a strong impression, or mental view, of a small black leather fold-over container which might have been a wallet or folder, or possibly the covers of a black leather notebook. I took it to be a wallet or folder. Then the impressions began to fade.

The next day, April 9th, I reported the entire incident to my friend *Helen Hoyt* (over the telephone) and she verified as accurate the physical characteristics of her former husband as they had been impressed upon me. Later on that same day *Helen* visited the home of the bereaved present wife† and there saw a mahogany chest of

* Pseudonym.

† All the circumstances surrounding the somewhat unusual relationship existing between the first and the second Mrs. *Hoyt* have been made clear to us; it has not been thought necessary, however, to clarify this in the presentation of the case.

drawers with brass handles—just as I described it to her. With some diffidence *Helen* persuaded the second Mrs. *Hoyt* to search in this chest of drawers for the folder indicated. A search was made and a black leather folder was found, as foretold, in one of the drawers with Mr. *Hoyt's* handkerchiefs. This folder contained an old photograph of *Helen*. The present Mrs. *Hoyt* had not known that the photograph existed, nor that there had been such a folder among her husband's things. *Helen* had no idea that the photograph was still in Mr. *Hoyt's* possession. In the same drawer a second black folder was discovered. It contained a codicil to Mr. *Hoyt's* will.*

Perhaps of further interest is the fact that it was generally believed that Mr. *Hoyt's* regard for his first wife had continued through the years. Furthermore, if the impressions I received may be considered as having their source in the surviving personality of Mr. *Hoyt,* the information given, though possibly painful to the second wife, constitutes an added and accurate character trait for the following reasons:

(1) Mr. *Hoyt* was a man of scientific training and would, I think, realize the importance of getting through information that could not easily be attributed to telepathy among the living. The existence of his first wife's photograph in the drawer with his handkerchiefs was just such a piece of information.

(2) I have learned that Mr. *Hoyt* was incisive, brusque, and not always considerate of the feelings of other people, being especially indifferent to them when he believed that they interfered with questions of scientific importance.

On the morning of April 9th I told my husband about my experience of the night before; I also told a friend, Jean Galer. Their testimony is appended. I should say that I told them all the details before there was any question of corroboration from the home of the second Mrs. *Hoyt.*

MILDRED C. HARRIS

Miss Harris inclosed corroboratory statements from Mrs. *Helen Hoyt,* from Mrs. Jean K. Galer, and from her husband. The latter prefers that his name should not appear and therefore signs his statement with initials only.

April 28, 1943

In verification of the episode recounted to you by Mildred Harris, I wish to state that on the afternoon of April 9, 1943, the day after the death of my former husband, Miss Harris reported to me on the

* In further correspondence we have learned that the codicil was of no importance or interest to the first Mrs. *Hoyt.*

telephone several impressions she had received the night previous. She had never, I am sure, seen Mr. *Hoyt,* yet she accurately described to me details of his physical appearance which had been impressed upon her.

As for the mahogany chest with brass pulls, it was only with some difficulty that I was able to ascertain that such a chest of drawers did in fact exist. I saw it, however, exactly as described to me by Miss Harris, when I visited the home of the present Mrs. *Hoyt* later on during the afternoon of the 9th. There was also found, as indicated, in a drawer with Mr. *Hoyt's* handkerchiefs, a black leather folder containing an old photograph of me. I had not known that this photograph was still in the possession of Mr. *Hoyt,* and his present wife did not know of the existence of either the folder or the photograph. In addition, there was discovered in the same drawer, in a similar black leather folder, the codicil to Mr. *Hoyt's* will.

Helen Hoyt

April 20, 1943

This is to state that on the morning of April 9, 1943, my wife reported to me on the telephone the details of an impression she had received the night before concerning Mr. *Hoyt,* who had died that day. Her account of the incident as reported to you is in all details the same as that given to me before the information was passed along to the first Mrs. *Hoyt* and later substantiated in each particular.

N. M.

April 20, 1943

On April 9, 1943, Mildred Harris stopped for a moment at my place of business and, knowing of my interest in the field of extrasensory perception, told me in detail, as in her own account submitted, of her experience of the night before concerning Mr. *Hoyt.* These details were told to me the day before they were corroborated by Mrs. *Helen Hoyt.*

On the evening of April 10th, Miss Harris, her husband, and Mrs. *Helen Hoyt* came to call on me at my home. At this time Mrs. *Hoyt* told us of finding, as a result of the impressions Miss Harris had told her about, the chest of drawers, the folder containing her photograph, and the other folder containing the codicil to her former husband's will. She also confirmed Miss Harris' impressions as to the details of Mr. *Hoyt's* physical appearance, these details normally being unknown to Miss Harris as she had never seen Mr. *Hoyt* during his lifetime.

JEAN K. GALER

Correspondence

Letter from Mr. Whately Carington to Dr. Murphy in regard to the results of the Society's 1941-42 "Long-Distance Telepathy Experiment."

One hundred and sixty-two percipients, many of them Members of the Society, took part in this experiment, which was carried out under the direction of Dr. Murphy. Five simple line drawings chosen by a random method from an already prepared population of 208 drawings were used on twelve experimental evenings (November 25, 1941 to February 24, 1942). Mrs. Dale, of the Staff, acted as agent, retracing and "concentrating" upon each of the five drawings in succession for a period of five minutes. The results of this experiment were sent to Mr. Carington in England for evaluation. Mr. Carington, of course, is well known for his experiments on the paranormal cognition of drawings. See *Proc.* S. P. R., Part 162, June, 1940 and Part 164, June 1941.

. . . I have now completed [January 5, 1943] all the work which at present seems necessary on your big experiment; that is to say, all the obvious tests and one or two less obvious. The results justify my gloomiest prognostications and are completely null, at least from the paracognitive point of view. Despite this null result, however, I do not regard the experiment as a failure, except in the restricted and local sense that it did not do what you hoped it would do. On the contrary, I regard it, oddly enough, as one of my major triumphs, for reasons which I must try to make clear.

I think you will agree, speaking in a general sort of way, that our attitude to any new phenomenon, and our conviction as to the genuineness of its occurrence, will depend largely on the extent to which we "understand" it, and in particular on the extent to which we are able to make predictions about it. I think we should feel suspicious in the presence of a phenomenon which occurred in a wholly sporadic fashion, so that we could never tell when it would "come off" and when it would not. In other words, we shall not feel happy about a new phenomenon until we have excogitated some kind of theory about it which will enable us to make reasonably successful predictions about its occur-

rence. And it is scarcely less important, though much less gratifying, of course, to be able to predict the conditions under which it will *not* occur; at any rate, a theory which predicted that the phenomenon in question would occur under *any* conditions could hardly be considered a theory!

Now, ever since you first told me of the projected experiment I have had doubts as to whether the modifications you introduced might not prove fatal to the occurrence of the effects you are looking for—see, for instance, my letter to you of July 27, 1942—and have felt that the plan of having the 208 potential originals prepared in advance was liable to lead to null results. The null result ensued, and I do not think it could have been predicted on any theory of paranormal cognition other than my own—except, of course, on the "theory" that paranormal cognition does not occur at all! But we know (humanly speaking) that paranormal cognition *does* occur, and that it occurs with drawings. I think that any other theory would say that the preparation of 208 potential originals in advance would make no difference, but that what would matter would be the "concentration" by the agent on the selected original at the appointed time. This, I think, would certainly be true on any "transmissive" theory, such as is commonly held by most people. But on my theory of paranormal cognition (Cf. *Proc.* S.P.R., Part 164, June, 1941, pp. 304 ff.), preparation ahead makes all the difference, as I shall try carefully to explain below.

Before doing this, however, I want to separate clearly two features of your procedure, each of which has had an adverse effect from one point of view or another, though of different kinds. The first of these is the use of a large number of pre-prepared potential originals, as already noted. The second is the fact that percipients were invited to draw up to 60 drawings (5 on each of 12 evenings), and did in fact produce anything from this number downwards. The first of these is, on my theory, liable to prevent anything interesting emerging at all; the second merely precludes the useful application of Catalogue scoring.* As

* Mr. Carington's method of Catalogue scoring will be fully explained in a paper of his to be published by the Society in the Fall of this year.

to the first feature of your procedure—that is, why the 60 originals actually used failed to give a significant result when hits on them are tested in a 12 x 12 contingency table by the ordinary Stevens method:

If my views are approximately correct—and I may say that almost every day brings forth confirmation of them in one way or another—these phenomena can be understood only by dismissing any type of theory which involves "transmission" from one localized mind to another localized mind across the intervening space. It is necessary to think instead in terms of the known facts or "laws" of association working on what is for this purpose no more than a *single mind*—call it the Joint Mind, or the Universal Subterconscious, or anything you like.

When the experimenter (who may or may not be the same person as the agent) draws an original X, the "idea" of X is ipso facto associated in that bit of the joint mind which we call "his" mind with the "idea" of the experiment. The term "idea" is used to denote that aggregation of images which enter or approach the field of consciousness when the object or situation concerned is thought of. This means that whenever the idea of the experiment is again evoked in the experimenter's mind by any future situation, the idea of X will be brought nearer to the focus of consciousness—i.e., will be more likely to enter the field—than it would be if the associative link had not been formed by drawing the original depicting X. That is perfectly straightforward associative psychology, and needs no particular elaboration.

But if we suppose that there is a "subterconscious" common to all individual minds in some degree, then this association of X with the "experiment" will have been formed *for all minds* by this act of drawing the original. Thus X will be more likely than it otherwise would have been to appear in the field of consciousness of any mind to which the idea of the experiment is presented. And this condition is manifestly fulfilled whenever a percipient sits down to do the experiment, with the result that any percipient is somewhat more likely to think of an X in these

circumstances than he would be if the experimenter had not drawn an X as an original.

Note that this theory short-circuits all difficulties about "rapport" and about why telepathy occurs only under certain conditions. Telepathy occurs perpetually under all conditions, and the reason why we are not as a rule conscious of it is simply because there is so much of it that it practically all cancels out—much as if we could hear all radio stations broadcasting at once, which would amount to the same as hearing none. As for "rapport" in the ordinary sense, there is none. But this will have to be discussed in more detail when we come to consider, as some day we must, the extent to which the subterconscious can reasonably be regarded as common to persons of widely different types and experience.

The really important point here is that we cannot think of the telepathic process as starting at the moment when the agent sits down to "concentrate" and the percipient to "receive." What matters is the forming of the associative links, and this, of course, is done when the experimenter draws the originals.

Thus we must think of the 208 potential originals as having been linked with the experiment *when they were drawn,* and the only advantages which the 60 officially used originals have over the other 148 are (a) the fact that the agent "concentrates" on them, and (b) the fact that since the 208 drawings were done some time previous to the start of the experiment, the associative links will have had some chance of fading and weakening.

As regards (a), I think it reasonable to suppose that the act of making an original drawing is likely to be a good deal more effective in impressing it on the mind of the drawer than is the rather vague process known as "concentrating"—though no doubt the concentrating serves to strengthen the links to some extent. As for (b), it seems pretty clear that astronomical time has nothing to do with the process except indirectly; indeed, it would be very odd if the rotation of chunks of rock in the sky had any direct influence on psychical processes. What matters is what I

have termed "relevant psychological time," which is a matter of the extent to which the images in the subterconscious are shuffled about, and the associative linkages and groupings altered. If you associate a group of ideas of any kind together (e.g., X, Y, Z, etc., with Experiment) and then do not think of the experiment for a month, the linkages will remain, one may suppose, substantially unchanged except in so far as they may be somewhat weakened by the strengthening of alternative links between X, Y, Z, etc., with other ideas in the course of everyday experience. Naturally, this last is positively correlated with astronomical time for most people, and that is how astronomical time comes indirectly into the picture; but it is easy to see that its effect may be small, and that there is no sort of guarantee in any particular case that the effect will be proportional to its clock-measured duration.

It follows from all this that, by having potential originals prepared in advance, one is automatically "putting them into circulation," so to say, in the relevant context, and that the 60 subsequently selected for "concentrating" on are likely to have only a small advantage over those not used. And as the paracognitive effect is pretty small anyway, an attempt to show that the 60 originals used are significantly differentiated from the 148 not used, or the 5 used on one evening from the 55 used on the other evenings, is a matter of looking for a kind of second-order small quantity which is very unlikely to succeed in the present state of our knowledge.

In your experiment, the person who made the original drawings may not have had much to do with the matter; Mrs. Dale was evidently the "culprit," so to speak, for there was every opportunity for the 208 potential originals to be firmly associated in her mind with the idea of the experiment*—and that is all that is necessary. The original drawer may have come into it in a minor and indirect way through a linkage with you, but we do not yet know enough

*LD helped to choose the words which were later illustrated by a City College student. She cut the drawings apart, placed them in envelopes, and so forth—thus being entirely familiar with all 208 potential originals.

to say so with certainty; anyway, it matters little in the circumstances.

In other words, if my theory is correct, we could hardly expect, granted the conditions of your experiment, any result of the kind looked for. Again I want to stress that what matters is not the *existence* of the drawings (originals) but the forming of the associations of the ideas of the objects depicted with the *idea of the experimental situation.* The world is full of drawings of objects associated in somebody's mind with something—*but not with the experiment.* If you had happened to pick up on the sidewalk an envelope marked "208 Sketches for Wanamaker's Winter Fashions Booklet," or the like, and had taken your 60 originals from that (and had then done a ten-trial experiment) I do not doubt that you would have had, on Catalogue scoring, a significant result.

Now for the second point—the fact that your methodology prevented the useful application of Catalogue scoring:

At one time I thought it possible that, even if the use of pre-prepared potential originals prevented the 60 selected from differentiating themselves from the remainder, it might still be possible to show that the whole 208 drawings yielded a score significantly above chance expectation. This, however, is not the case, and the reason is easy to see. My own Catalogue embodies the drawings of 741 percipients doing 10 drawings each (more accurately, an average of 11.42 each), and naturally can only be used with assurance to assess the performance of percipients doing an equal, or nearly equal, number of drawings. Your percipients sent in about 28 responses each, on an average. We cannot properly allow for this by arguing that each of your percipients is 2.8 times as likely to draw any given object, X, as a percipient of my Catalogue, and that therefore your expectation of X-drawing percipients is 2.8 times the Catalogue figure, because if the percipient has any sense he will realize that if an X has been used as an original once it will not be so used again, and that there is nothing to be gained by drawing the same thing twice. Of course, a fair number of percipients do not think of this, so that in

practice there is a certain amount of replication. For the most part, however, things are likely to work this way, so that the true expectation for your percipients, for any original—notably common originals liable to be drawn early—will be less (though I do not know how to determine how much less) than the value calculated from the Catalogue on the fiction that you had 2.8 times as many percipients as you actually had. Equally, of course, it would be misleading, in the opposite direction, to treat your percipients as if they had only done an average of 10 originals each.

It is accordingly not surprising to find, as we do, that when the responses to the 208 potential originals are scored by the Catalogue procedure they yield a significant negative result—i.e., when I pretend that there were 2.8 times as many percipients as there actually were. This is, of course, a purely arithmetical matter which has nothing to do with paranormal cognition as such, and it serves only to illustrate the impracticability of using a yard-stick based on data obtained by one method to assess data obtained by another method.

It might, it is true, be argued that, if paranormal cognition had been operative, we ought to obtain a positive result by scoring the *first ten* drawings done by each percipient (where he has done so many as ten, or more) against the 208 potential originals by the Catalogue method; and I think that if such a scoring were to produce a significant positive result it would be admissible. But I do not think that the opposite holds, for the psychological situation of a percipient asked to do 60 drawings and actually proposing to do so (we may reasonably assume), or as many as he can, is by no means the same as that of the percipient asked to do only 10 and knowing that only ten originals will be used. In particular, it seems reasonable to suppose that, with the prospect of time to play with, so to say, he may indulge in a greater degree of picking and choosing—deferring, perhaps, to some future occasion, in the hope of reinforcement or whatever, some of the images that first present themselves. In other and more general words, in

so far as the conditions are different you cannot expect the same things to happen. Consequently I have not thought it worthwhile to undertake the very considerable labor of doing this test. It is rather recondite at best, and, if it gave null results (as inspection and the working of a small sample suggest), it could so easily be explained away on the above lines as to be without meaning.

In conclusion, I repeat that I do not consider the experiment to have been a failure at all, from my point of view, or indeed from any other point of view except the most local. Successfully to predict that a given effect *will not* occur under certain conditions may, I think, quite legitimately be claimed as constituting just as good confirmation, in its own way, of one's guiding theory as successfully to predict that it *will* occur under other conditions. A significantly positive result in your experiment would, at least, logically have required a fairly drastic revision of my theory in order to account for it.

Review

APPARITIONS: being the seventh Frederic W. H. Myers Memorial Lecture, by G. N. M. Tyrrell. Society for Psychical Research, London, 1942. 123 pp. 3s. 6d.

It has been a long time since we have had a mature, scholarly, and systematic study of apparitions; indeed, to find anything comparable to Mr. Tyrrell's paper we would have to go back to Mrs. Sidgwick's article* in the *Proceedings* (1922), and even there the author restricted herself to certain aspects of the present problem. The Tyrrell monograph aims, in accordance with the high design of the Myers Memorial Lectures, to achieve much more than would a mere collection or systematization of recorded cases; it presents a psychological theory of apparitions—visual, auditory, and tactile—and makes pertinent suggestions for additional further inquiries.

The cases cited cover a large number of the old familiar "standbys," but they are chosen, as by a geological or botanical collector, to document the salient *types* of data which have to be reckoned with on any theory. With negligible exceptions, they are S. P. R. cases; they are *waking* cases; and they are—except when, for purposes of comparison, other features are called for—well authenticated veridical cases. Precognitive cases (e.g., those referring to future catastrophes) are excluded. We are dealing almost entirely with two large classes of evidential apparitions: (a) Those suggesting agency of the living or the dying, and (b) those suggesting agency of the deceased. Mr. Tyrrell's own fourfold classification comprises: (1) Experimental cases, as when a man successfully wills that his form appear to a distant friend. (2) Crisis-cases, as when an aviator, shot down in France, appears to his sister in India. (3) Post-mortem cases, as when the form of a man's deceased father appears as if to reproach his son, the son's wife also seeing the apparition. (4) Ghosts, which persistently appear in specific places, and to a variety of persons who "see" them simply by virtue of living in, or visiting, those places.

In the reviewer's judgment, by far the most important original contribution of the monograph lies in its systematic study of the psychological attributes of apparitions. It deals with those common characteristics which make them a distinctive class, and also with the variations which permit establishment of sub-classes. The result is

* Phantasms of the Living. An Examination of Cases of Telepathy between Living Persons printed in the "Journal" of the Society since the publication of the book "Phantasms of the Living," by Gurney, Myers and Podmore, in 1886, *Proc.* S.P.R., Vol. XXXIII, pp. 23-429.

to demonstrate ***consistencies*** or ***uniformities*** running through an enormous range of human experiences—a basis for a Linnean system for establishing species. Here an ingenious device is used to classify the general "type." Just as you would have little difficulty in approximately indicating all the characteristic qualities of a rose or a horse, though no *one* rose, or horse, had *all* the qualities, Tyrrell defines (pp. 54-5) the observed qualities of the "perfect apparition," which is a composite of the attributes of the best authenticated examples (any one apparition having several, but not all of these attributes). He compares this "perfect apparition" with a normal human being, and finds many points of resemblance. Some of these may be summarized as follows:

(1) Both figures would stand out in space and would appear equally real and solid. (2) As regards distance and perspective we would detect no difference between the apparition and the living person. (3) Both figures would be seen clearly or otherwise in relation to the amount of light. (4) Both figures would obscure the background. (5) Sounds, such as breathing and rustling of clothes, would be heard on approaching the apparition. (6) The apparition would probably behave as if aware of our presence. If it placed its hand on our shoulders we would feel an ordinary human touch. (7) It might speak to us, but we would not be able to engage it in any long conversation. (8) We would see the apparition reflected in a mirror. (9) Both figures would cast shadows. (10) On shutting our eyes or turning away our head, the apparition would disappear just as a real figure would do; on reopening our eyes, we would see it again. (11) In addition to its clothes, the apparition might carry a cane or other accessories, and it might be accompanied by a dog or by another human being. (12) The apparition might pick up any object in the room, or open and close the door. We would both see and hear these objects moved.

Points of difference between an apparition and a normal human being are brought out, among them the following:

(1) In the presence of the apparition we might feel a sensation of cold. (2) If we tried to take hold of the apparition, our hand would not encounter any resistance. (3) An apparition would not leave footprints, though its steps can be heard. (4) It is inferred that if a photograph were taken of both apparition and living human being, only the human being would come out. And only the sounds made by the human being would be recorded on sound-recording apparatus. (5) The apparition might disappear suddenly, or vanish into the wall. (6) It might become slightly luminous, and small details of it might be visible to us when we were so far away from it that such details could not normally be seen.

After disposing of the physical theory of apparitions, and accepting a psychological interpretation, Mr. Tyrrell proceeds to formulate a comprehensive view based upon (a) a systematic modern statement of the psychology of normal perception, and (b) an hypothesis regarding telepathic rapport between personalities.

The former will be familiar to those of our readers who have seen Mr. Tyrrell's *Science and Psychical Phenomena;* there as well as in the present monograph we find an articulate statement of the view that the "solid objects" with which an observer peoples space are in fact "constructs" based on sense-data which, immaterial as they are, are so ordered as to provide the conviction that they are "out there," parts of a material three-dimensional world. (This is not Berkeleyian idealism, for a real external thing-in-itself is accepted as acting upon our bodies; but the only *properties* we know are sensory properties.) The theory is developed with full benefit of modern experimental psychology, and of psychoanalytic and Gestalt principles, to show that the sense-data are thrown into a form meaningful to the individual perceiver only by virtue of a very complex quasi-automatic process. The unconscious organizing tendencies of the individual, based on his ceaseless need to deal with sense-data as clues to the handling of objects in space, throw each new mass of sense-data into organized relevant form; thus an idea, albeit one not present in consciousness at the time, can clothe sense-data with full external reality, e.g., one sees what one expects to see.

To explain apparitions, the unconscious dynamics of both agent and percipient require analysis. Mr. Tyrrell proceeds to "compare the consciousness of the agent with the author of a play, and that 'something' within him which works out the idea in dramatic form to the 'producer.' Further, the 'something else' within him which expresses this drama in the sensory form of an apparition" is compared "to the 'executor' or 'stage-carpenter' of the play . . . The apparitional drama is quite clearly in most cases a joint effort in which the producers of both agent and percipient take part" (p. 72). Of course, the construction of the drama is conceived to take place beneath the levels of consciousness, and Mr. Tyrrell describes the processes involved as being "very different from anything which takes place in the physical world and different from purely mental processes as well." He says (p. 73), "There is a good deal in the apparition which suggests consciousness and there is a good deal which suggests automatism. The truth is that we are dealing with something between the two extremes of consciousness and mechanism. We are dealing with something which is to a certain extent *like* an idea, and, at the same time, to a certain extent *like* a pattern. I have thought it simplest, therefore, to call it an 'Idea-pattern.' This

idea-pattern is the dramatic production of the agent's idea; while the apparition itself is the sensory expression of the idea-pattern."

An apparition, being a non-physical reality and hallucination, must accordingly be structured by the perceiver as if it were an ordinary external object. Hence it obeys perfectly the psychological laws of perception; it *imitates* ordinary percepts. Evidence is produced to show that the apparition obeys familiar optical laws, disappearing when the eyes are closed, giving rise to an image in a mirror, etc. The apparition, then, is the way in which the perceiver projects an idea-pattern into his ordinary space; and if the hallucination is veridical, the initial impact is telepathic, depending on the unconscious dynamics of both agent and percipient.

The general viewpoint here will not be entirely unfamiliar to those readers of Myers who have followed the psychoanalytic conception of the unconscious structure-giving properties of the mind; but the present statement is probably the most complete formulation of the relation of telepathy to the unconscious which has yet been offered.

Mr. Tyrrell gives a great deal of attention to the contrasting views of Gurney and Myers regarding collective veridical apparitions. Gurney believed that a single percipient telepathically influences the others present so that they share his experience. Myers held that the apparition actually *invades* the space—yet not the physical space —of the percipients. Mr. Tyrrell easily shows that neither view is completely satisfactory, and offers a conception of his own to the effect that other persons present when an apparition is seen are *relevant* to it. He says (p. 79): "Whatever would be the behaviour of the physical figure of the agent under existing circumstances is exactly reproduced. This thorough imitation of the physical event, which forms the *motif* of the theme, accounts, I believe, for collective percipience. Other people standing near the place where the apparition is seen would, if the apparition were a flesh-and-blood person, certainly see him. Therefore, to carry out the scheme thoroughly, they are so psychologically acted upon as to see the apparition and to see it appropriately according to their positions and distances. Their 'producers' and 'stage-carpenters' must, in fact, be got to collaborate with those of the agent and principal percipient. Thus, collective percipience depends on the physical positions of the percipients, *not because physical facts have anything to do with it,* but because their physical positions bring them into the scheme of the idea-pattern. In a word, it may be said that the explanation of collective percipience of apparition lies . . . in the fact that spectators, by their physical presence, become *relevant* to the theme of the apparitional idea-pattern and, because relevant, are drawn into it."

This theory of "relevance" is interesting and is a step in the right direction; another hypothesis, however, is possible. What Mr. Tyrrell *almost* says, again and again, is that apparitions are a part of the *psychological space* of the observer. Now psychological space, *space as we experience it in everyday life,* as has been experimentally shown, is non-symmetrical, fluid, expressive of individual attitudes; it is essentially like all things of the mind. Now, what makes the phantasm of A, thinking intensely of his friend B, appear to B is in part the fact that *A's psychological space*—as he thinks of B and of B's surroundings—*overlaps B's psychological space.* Further, telepathy seems to depend to some extent on *common features* in the minds of agent and percipient, for we are bound together by a network of related associations. Hence, since the psychological space of B, C, and D, who are together, is also somewhat similar, A's thought of B may well provide a collective apparition for all three.

It must in candor be added that neither Mr. Tyrrell's conception of relevance, nor the reviewer's comments on psychological space, does anything much to remove the essential mystery of telepathic contact between persons, except in so far as they emphasize that physical space, having nothing to do with the matter, does not in fact "separate" persons. The reviewer is, moreover, puzzled that Mr. Tyrrell has not referred here to Whately Carington's "one big mind" theory, with which he is presumably fully familiar. This is an ambitious attempt to determine the *modus operandi* of psychological contact between minds. Mr. Carington's theory, we understand, is soon to see the light in fully articulate form; perhaps Mr. Tyrrell felt that it would be ungracious to refer to it before its publication.

Mr. Tyrrell has probably added to his difficulties, rather than lessened them, by excluding precognitive apparitions. Two mysteries are not necessarily worse than one if they throw light upon one another, and Bergson and Einstein alike make it seem probable that we cannot successfully attack the problem of space without facing the problem of time.

On the other hand, Mr. Tyrrell's discussion of clairvoyance is highly pertinent and helpful. He points out that the evidence for clairvoyance (the direct perception of physical objects by other than sensory means) is far from satisfactory; that, indeed, because of the possibility of precognition of the experimenter's mind as he checks the results, there are many difficulties in the way of devising an experiment which would test the hypothesis of pure clairvoyance. He discusses, however, the manner in which his own apparatus* for testing supernormal perception might be adapted to test pure clair-

* For a description of this apparatus, see *Proc.* S.P.R., Vol. XLIV (1936-37), pp. 99-168. Also *Journal of Parapsychology,* Vol. II, June, 1938, pp. 107-118.

voyance. This apparatus might easily be used to score *total* hits, the experimenter never knowing the target for any individual call. The results of such an experimental situation, if extra-chance, could not be accounted for in terms of precognition and would indicate the existence of a true clairvoyant faculty.

As regards the bearing of apparitions upon the survival problem, Mr. Tyrrell's argument is straightforward. Most apparitions of the living emanate from the personalities of those whose forms are seen. In most post-mortem and ghost apparitions, we must, if we are consistent, attribute the *initiation* of the impulse to the deceased, though of course the form which it takes in the percipient's experience is, as usual, due in considerable measure to his own personality. It is, moreover, emphasized that we do not get far by constantly asking ourselves whether a given experience settles the survival question. We do not yet know enough to put our questions to Nature in an answerable form. But apparitions do a great deal to *enlarge our understanding of human personality,* and hence may indirectly yield data which will in time clarify our questions about survival.

Towards the end of the paper there is much trenchant discussion of possible experiments upon hallucinations, normal and supernormal, especially through the aid of hypnosis, with a view to a better understanding of the unconscious dynamics by which an idea gives rise to the externalized, dramatized apparition. In particular, a plea is made for an attempt at the experimental production of *collective* apparitions. It is certainly to be hoped that skilled hypnotic operators with suitable somnambulistic subjects will heed this plea, for such an experimental program is long overdue.

If it be regretted that all the maturity and wisdom brought into focus in this paper, published in 1942, still leaves us not so very far from where we were upon the publication in 1886 of *Phantasms of the Living,* it must be pointed out that from Newton to Einstein was 250 years, that the number of workers in our field is small, that fresh materials different from the old are few, that reluctance to attempt the present type of incisive, systematic thinking still prevails, and that a blind fear of experimental methods, which are somehow conceived to spoil and deflate reality, persists almost unabated today. It is earnestly hoped that the three salient features of the present monograph will become more and more characteristic of psychical research: its direct confrontation of Nature on her own terms; its patient and systematic ordering of the data into intelligible classes with the use of bold, yet flexible, theory, and its specific recommendations for the use of experimental methods by which theories can be tested, and principles finally established.

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